The Craft of International History: A Guide to Method
Roundtable *Review*

Reviewed Works:

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Roundtable Editor: Patrick Finney
Reviewers: Antony Best, John Ferris, Petra Goedde, Geoff Roberts

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h-diplo ROUNDTABLE *Review*

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Introduction by Patrick Finney, Aberystwyth University

Marc Trachtenberg's *The Craft of International History* makes a distinctive contribution to the literature on international history, and it has justifiably attracted considerable comment since its publication.[1] Various other texts provide overviews of competing interpretive approaches or recent historiographical developments within the field, but there is nothing which offers such detailed practical advice on methodology as this 'guide to how historical work in this area can actually be done' (vii).[2] As one of the most eminent and accomplished international historians in the United States, with extensive publications on American and European international relations across the twentieth century and on history and strategy, Trachtenberg is well placed to offer such guidance. He attests in his preface that this long-gestated book is also a product of his experiences over many years delivering methodological instruction to undergraduate and graduate students. Recently he has generously made some of the course literature he developed for that purpose available online as part of the Internet supplement for his 1999 book *A Constructed Peace*, and this new text both codifies and considerably expands on that material.[3] That Trachtenberg is also well-known for his trenchant views on the health, status, and orientation of our discipline – he played a leading role in the foundation of The Historical Society in 1998—lends this book additional significance as an intervention in debates about the practice of international history.

Patrick Finney *teaches international history in the Department of International Politics at Aberystwyth University. He has a BA in International History and Politics and a PhD from the University of Leeds. His publications include edited volumes entitled* The Origins of the Second World War *(London: Arnold, 1997) and* Palgrave Advances in International History *(London: Palgrave, 2005). His monograph* Remembering the Road to World War II: International History, National Identity, Collective Memory *is forthcoming.*

The Craft of International History is explicitly intended to instruct both international historians and political scientists, since Trachtenberg believes that the two have much to learn from mutual dialogue. On the one hand, he argues that international historians can benefit from thinking rigorously about methodological and theoretical issues, and in particular that their historical work will be improved by engagement with the conceptual

[1] See, for example, the earlier roundtable review entitled 'International Relations Theory and Diplomatic History', *Historically Speaking*, vol. 8, no.2, 2006, 11-21.

[2] See, for example, Patrick Finney (ed.), *Palgrave Advances in International History* (London: Palgrave, 2005); Michael J. Hogan and Thomas G. Paterson (eds), *Explaining the History of American Foreign Relations* (Cambridge: Cambridge University Press, 2004, 2nd edn); Robert D. Schulzinger (ed.), *A Companion to American Foreign Relations* (Oxford: Blackwell, 2003).

[3] Marc Trachtenberg, *A Constructed Peace. The Making of the European Settlement, 1945-1963* (Princeton: Princeton University Press, 1999). These materials can still be accessed via Trachtenberg's website at http://www.polisci.ucla.edu/faculty/trachtenberg.

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and theoretical apparatus of International Relations (IR). On the other hand, he avers that political scientists can enrich their own work by cultivating a more sophisticated historical sensibility, since only thus is it possible to connect theory up with reality, and to get 'a real sense for how things actually work' in international politics (45). These claims are not uncontentious in either camp, of course, and while Trachtenberg's chief target audience is younger scholars just beginning their training, there is much in his book that will provide food for thought even for much more experienced practitioners.

Trachtenberg begins by discussing the theoretical foundations of historical inquiry, canvassing recent developments in both the philosophy of history and the philosophy of science; his view is that it is the latter body of writing rather than the former that is 'of real value to the practicing historian' (1). In chapter two, he expounds his views on the relationship between international history and IR. Insisting that 'a historical interpretation has to have a conceptual core' and that historians have to draw on 'a kind of theory …, a certain sense for how things work' (30), he demonstrates how IR theory can assist historians in framing questions, developing research strategies and generating interpretations. Simultaneously, he asserts that political scientists should not—as they too commonly do—simply treat history as a store house of facts to be deployed in theory testing, but should rather be prepared to study historical episodes in more depth, so as to 'develop the kind of sensibility that makes intelligent judgement possible … about how things work in the real world' (44-45). Lest he be thought to be demanding that theorists actually become historians, Trachtenberg then goes on in chapter three to illustrate how political scientists or historians wishing to orientate themselves in a new area of historical scholarship can reach fairly solid conclusions reasonably quickly through the critical analysis of secondary sources. After preliminary practical guidance on conducting literature searches, he stresses the need to identify the essence of a text's argument, its architecture or key claims, its internal logic, and the adequacy of the evidential base on which it rests. He then demonstrates how this method can work in practice by case study critiques of canonical works by A. J. P. Taylor, Fritz Fischer, and Richard Neustadt.

Trachtenberg's fourth chapter moves beyond the analysis of individual texts to show how similar methods can be used to get to grips with whole bodies of historiography, thereby to develop one's own interpretation of some complex historical problem. He does this through a very extensive case study discussion of the origins of the Pacific War in 1941. Trachtenberg identifies the key explanatory problems within this larger historical issue, specifically the nature of American attitudes towards the developing global crisis of late 1941; the intentions of Roosevelt's policy towards Japan; the nature of Japanese policy towards the United States; the linkages between American policy in the Pacific and towards Europe; and how these elements fit into the larger story of the course of international politics from 1939 to 1941. Through identifying the core claims, internal logic and evidential base of important texts, weighing competing interpretations against each other and bringing to bear some evidence from primary sources, Trachtenberg elaborates his own interpretation, a variant of the 'back door to war' thesis in which 'Roosevelt used the situation with Japan as a way of bringing the United States into the European war' (131).

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This chapter is by far the longest in the book, comprising almost a third of the two hundred pages of substantive text, but this is in line with Trachtenberg's strategic purpose: in seeking to illustrate the thought processes behind the evolution of an interpretation, he has deliberately left 'a lot of the scaffolding up' (viii).

If these parts of the book devote considerable attention to larger theoretical, methodological, and interpretive issues, the remainder of it is relatively more concerned with the 'nuts and bolts' business of conducting archive-based international history. Chapter five discusses how to go about working with documents, exploring—again, with copious practical examples—the kinds of materials likely to prove helpful, the need to approach them with meaningful questions in mind, and problems of reliability, positioning, selectivity, and gaps in the record. Chapter six offers detailed guidance on how to go about starting a project, and particularly on how to use a variety of electronic and printed databases to identify the key secondary works and useful primary sources relative to any given topic. Chapter seven moves on to the writing up phase, with advice on formulating an argument, imparting structure to exposition, the integration of evidence and analysis, and the process of refining texts through multiple drafts. Finally, the book concludes with two very extensive appendices, the first essentially bibliographical and concerned with the identification of relevant scholarly literature, the second dealing with access to published and archival primary sources.

The reviewers gathered together here concur that Trachtenberg's book is a very valuable and innovative contribution to the international history literature, and that graduate students—and, indeed, faculty—will profit greatly from his practical methodological advice. However, they also express a series of reservations about his over-arching vision of the discipline and the validity or robustness of some of his more specific claims.

1. Trachtenberg never explicitly discusses how he construes the terrain of international history, but his treatment implies that he regards it as essentially equivalent to the history of high politics and diplomacy. Several reviewers lament that this 'traditional' vision conveys a misleading and outdated impression of the breadth and diversity of practice within the contemporary discipline, since it not only marginalises recent developments in strategic, intelligence, and economic sub-fields but also the whole gamut of work flourishing under the rubric of 'culturalism'. Each of us is entitled to form our own judgement as to what constitutes the most significant work in the discipline, of course, but several reviewers suggest that Trachtenberg's chosen stance limits the applicability of his insights and the utility of his book as a classroom introduction for tyro scholars. Does Trachtenberg then—despite the confident definite article which heads his book's title—in fact only offer guidance on how *some* historical work in this area is actually done?

2. Trachtenberg insists that international historians need to bring to bear conceptual and theoretical thinking in their work, and that IR theory can provide profitable stimuli here, but he consistently privileges one particular theoretical approach,

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namely that of realism. Indeed, his whole text is suffused with a sense that realism provides the crucial commonsense insight into 'how things work' in international politics (30). It may well be, as Geoff Roberts argues, that in partaking of a form of 'soft realism' Trachtenberg simply reflects the dominant mode of thinking within the discipline. But Antony Best and John Ferris both claim that this commitment leads Trachtenberg into making some dubious claims about causation in his case studies, while Petra Goedde asserts that it blinds him to more complex understandings of 'how states and cultures interact'. More problematically, Trachtenberg consistently talks of the need to deploy theory sceptically and selectively, and of the necessity to avoid imposing theory on the evidence, and yet he seems to have made a fairly whole-hearted investment in realism (puckishly, one might say he has indeed 'fall[en] in love with a certain way of looking at things' (33)). Such a commitment can certainly be defended, but this is not done explicitly within the text, where readers are persistently led to believe that 'things more or less *have* to work' in the way realism dictates (35).[4]

3. A related criticism is that Trachtenberg defines 'theory' in too restricted a fashion. When discussing IR theory, he draws his examples exclusively from mainstream, 'hard' American political science—referring, for example, to 'offense-defense' theory or the 'relative gains' literature—to the neglect of other approaches—such as constructivism—which also have much to offer international historians. Moreover, as Petra Goedde argues, he does not consider the wider range of social and cultural theories which have recently been deployed to fruitful effect by historians in many different fields. Of course, no author can tackle the totality of any subject within the spatial constraints of a single text and so all will prioritise some approaches over others, but these choices do have normative implications which the student target audience might not readily discern. Moreover, if international historians are to remain engaged with wider intellectual debates in the academy and society, do they not need to be equipped to respond when they encounter these alternative approaches in their reading?

4. Trachtenberg's concentration on high politics and his realism both conduce to an attitude towards sources that troubles some of the reviewers. His conception of elite policy makers as super-rational actors, enjoying privileged access to 'the realities of international politics' (153), leads him to prioritise traditional political and diplomatic archival materials ('you should start at the top and proceed from there' (142)) and to downplay the potential contribution of open sources. While the former obviously remain crucial for many topics in international history, Antony Best observes that exploration of the latter is essential to illuminate the context and constraints within which policy makers operated. Pushing this point further, Petra Goedde argues that culturalist international historians have made fruitful use of a whole range of non-archival sources to illuminate the wider systems of cultural

[4] Trachtenberg talks more explicitly of his engagement with realist theory in his 'Rejoinder' in the roundtable 'International Relations Theory and Diplomatic History', 20-21.

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values and assumptions that condition the exercise of political power, in tune with their more expansive conceptualisation of international relations. Is it not the case that what constitutes the 'key bodies of evidence' (87) is never self-evident, but rather depends on the theoretical and interpretive assumptions that historians are bringing to bear?

5. The reviewers devote considerable attention to Trachtenberg's extended case study of the road to Pearl Harbor. Antony Best and John Ferris both argue that his treatment is flawed in its specifics because he has failed to follow his own excellent bibliographical advice on the location and identification of the key works on a given topic. Best argues that Trachtenberg has overlooked some of the most important contributions to the literature, and Ferris concurs, lamenting particularly Trachtenberg's apparent unfamiliarity with the recent—and highly germane—literature on the intelligence dimension. The two reviewers are also critical of the reliability of Trachtenberg's preferred authorities for explaining the nature of Japanese policy. That Trachtenberg has based his analysis on an idiosyncratic selection of the literature and skewed reading of the evidence arguably undercuts the plausibility of his 'back door to war' interpretation. Moreover, Best and Ferris also imply that Trachtenberg's interpretive preferences here are not unconnected to his commitment to realism. For Ferris, Trachtenberg errs in his interpretation of Japanese policy because of his explicit belief that no 'rational' state in its position would have contemplated war with the United States. From this, given Trachtenberg's over-arching assumption about the rational and calculating nature of statecraft, a particular characterisation of American policy has to follow to account for the outbreak of war. Best similarly argues that the 'back door to war' thesis over-simplifies a complex historical reality, downplays the significance of ideology, misperception, and unexpected contingencies, and is withal far too determinist and neat and tidy.

6. Moving beyond the specifics of Trachtenberg's interpretation, this case study also raises some general questions about his prescriptions for the practice of international history. First, do the alleged deficiencies of Trachtenberg's source base here throw some doubt on the general applicability or adequacy of his advice on how to conduct preliminary bibliographical searches? Second, is tension not generated as Trachtenberg sets out to demonstrate how to develop an interpretation—how, in other words, to do international history—but concludes with the adoption of a position at odds with the dominant scholarly consensus? Trachtenberg's rhetoric suggests an impatience with interpretive pluralism: he raises the stakes when he suggests that when authors disagree, the key question we need to ask is 'who's right?' (79). By his own lights, then, we are led to conclude in this case that either he has erred or the 'serious writers' (107) with whom he disagrees have done so. Student readers are likely to be left slightly confused here, as by his assertion that in case of disagreements it is possible to 'get to the bottom' of the issue simply by looking 'in a very targeted way at the empirical evidence'

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(107), as if the eminent authorities on the other side of the debate had not already done this. Were Trachtenberg simply presenting his interpretation as but one amongst many—as the kind of reading that an historian inclined towards realism might favour—then this would be less problematic, but his treatment rather implies that he has found a privileged method for penetrating through to the actual logic underlying these events.

7. Although it was clearly not Trachtenberg's intention to write a treatise on historical theory, some of the reviewers regret that he did not develop his thinking here further. Petra Goedde criticises his attachment to 'fact-and-documents based positivism', charging that he simply brackets the epistemological challenges entailed in historical work. Does Trachtenberg then fail to deal adequately with the fact that historians, even when adhering scrupulously to disciplinary conventions about the handling of empirical evidence, inevitably generate 'multiple and at times contradictory interpretations of the past' because of the divergent conceptualisations and theoretical approaches they bring to bear? In this respect, it is perhaps noteworthy that in his critical analysis of secondary texts he directs our attention to their explicit argument, architectural structure, internal logic, and supporting evidence, but relatively neglects the issue of their more profound underpinning assumptions. Equally, it might have been helpful if in his chapter on working with documents he had considered some historiographical case studies where historians have reached conflicting conclusions on the basis of the same empirical evidence.

8. Geoff Roberts is much more sympathetic to Trachtenberg's theoretical position, though he feels that Trachtenberg is too dismissive of R. G. Collingwood and that his historiographical practice is actually more in tune with Collingwoodian insights than he is prepared to admit. Roberts also regrets that Trachtenberg does not devote more attention to the issue of narrative. Roberts is no partisan of Hayden White, but he observes that other understandings of the nature and function of narrative are available and that Trachtenberg neglects these with his turn to the philosophy of science literature, predominantly concerned as it is with the framing of potentially answerable questions and the rational development of 'a powerful deductive system' (27). Trachtenberg's insistence on imparting 'a sense for a certain element of *necessity*' (185) in composing a work of history is slightly misplaced for Roberts, who would emphasise instead contingency and 'the *possibility* of human choice'. This suggests that it is not necessary to subscribe to White's notion of narration as a 'poetic act' to think that there is more to say about the transformation of data into narrative than Trachtenberg offers in his final chapter.

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Review by Antony Best, London School of Economics and Political Science

For a historian to write about how students should approach historical research is undoubtedly a brave venture. Many would instinctively shy away from such a task, for the one thing that can be guaranteed in engaging in this kind of project is that your peers will have strong views that do not necessarily accord with your own and that criticism will duly flow. It is therefore extremely heartening that an historian as eminent as Marc Trachtenberg has decided to bite the bullet and provide students with a wide-ranging methodological guide to approaching international history, for a well-crafted and thoughtful book like this is desperately needed. This book is destined to be of great utility to teachers and students alike, both as a guide and a starting point for debate, and it is to be greatly welcomed.

Antony Best *studied for his undergraduate degree at the University of Leeds before moving to London where he studied for his PhD at the London School of Economics. He joined the LSE as a Lecturer in 1989 and is currently Head of Department and Senior Lecturer in International History His recent publications include* British Intelligence and the Japanese Challenge in Asia, 1914-1941 *(Palgrave/Macmillan, 2002); 'Economic Appeasement or Economic Nationalism?: A Political Perspective on the British Empire, Japan and the Rise of Intra-Asian Trade, 1933-37'* Journal of Imperial and Commonwealth Studies*, 2002, vol.30, no.2, and 'Ohitsu-gaiko kara mita Nichi-Ei kankei 1919-1941' [The Royal Dimension in Anglo-Japanese Relations, 1919-1941] in Y. Ito & M. Kawata (eds),_Nijuseki Nihon no tenno to kunshusei-kokusaihikaku no shiten kara 1867-1952_ [The Emperor and Monarchy in Twentieth Century Japan from an International Perspective] (Yoshikawa Kobunkan, 2004). He is also a co-author of* An International History of the Twentieth Century *(Routledge, 2004). [co-written with J. Hanhimaki, J. Maiolo and K.E. Schulze].*

Any guide to methodology will, of course, be influenced by the author's own attitude towards their discipline and by the actual research that they have carried out. In Trachtenberg's case we have an historian whose present post is Professor of Political Science at UCLA and who has specialized in the history of American-European political interaction in the twentieth century, but who has also shown an interest in strategic studies. The upshot is that this book is written from the perspective of someone versed in what might be termed 'traditional' international history, but who displays a distinct sympathy towards the work carried out in International Relations (IR). Indeed, Trachtenberg describes the book as being partly intended to introduce political science students to historical research. In addition, it is clear that in regard to IR, he leans towards the realist perspective. For example, he declares unequivocally that 'the key point … is that international politics is about conflict' (141) and virtually all of his examples duly allude to crises and strategy.

This will, of course, be like a red rag to a bull for those international historians who believe that the discipline is about more than the study of war origins and consequences. Moreover, it may only provoke a disapproving sigh from those critics within the historical profession who believe international history to be irretrievably conservative. The real issue though is whether this traditional perspective in any way short-changes students in

terms of how they ought to approach topics or handle the key research sources. On the whole, this is not the case. In many of the areas that Trachtenberg covers, such as conceptualizing and writing about one's project or using library and archive finding aids, his guidance is wise and it is easy to adapt his detailed advice to meet one's own needs. He often states the obvious, but teachers of history will be aware from their experience of supervising student dissertations that this is often exactly the kind of advice that is needed.

One area, however, where his guidance might cause concern relates to his understanding of politics and his advice on open sources, such as newspapers and the records of legislative assembly debates. Following his realist inclinations, Trachtenberg's primary interest is in the activities and thoughts of the state's 'key people' (143), in other words politicians, diplomats and senior military personnel. These figures, he argues, exist in a rarefied world where they are exposed to 'the realities of international politics' (153). As such, it is in official documents, and perhaps in private papers, that one gets to understand how they think and how policy is made. Open sources, he contends, are less useful, for what one finds in this material is largely politicians engaging in rhetoric designed to win over public opinion.

This is good advice up to a point and it probably reflects the consensus among international historians as a whole, but it does underplay the contribution that open sources can make to a project. To put any crisis in context, to learn what would be politically acceptable, risky or even suicidal, it is surely necessary to know something about public opinion, whether elite or popular, and this can best be gathered from open sources. For example, one of the most revealing books on British appeasement of Germany is Nicholas Crowson's *Facing Fascism: The Conservative Party and the European Dictators, 1935-1940* (London: Routledge, 1997), which outlines the degree to which Tory opinion restricted the foreign-policy choices open to the Baldwin and Chamberlain governments. In addition, political speeches should not be dismissed as merely airy rhetoric, for they can illuminate both individual and societal beliefs and values. Philip Williamson in his *Stanley Baldwin: Conservative Leadership and National Values* (Cambridge: Cambridge University Press, 1999) uses Baldwin's speeches to explain his reaction to the revival of German power in the 1930s and in doing so sheds light more broadly on British society as a whole. Open sources can therefore be very useful, and it is a matter of regret that many international historians seem to be reluctant to engage in such research.

Aside from his approach to the political process, the other major problem with the book is the way in which Trachtenberg in chapter four presents a case study of American entry into the Second World War in 1941. This exercise is designed to demonstrate how one can approach familiar, well-covered topics, but a number of difficulties arise, both with his methodology and his analysis when he looks at events in the Pacific. In regard to his methodology, it is a shame that Trachtenberg does not refer back to the important advice he offers in chapter three about how historiographical essays can be used as a way of directing reading. It would, for example, have been useful in chapter four if he had engaged with Michael Barnhart's excellent historiographical review of the origins of the Pacific War

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and indicated how this essay helps one to identify and assess the key works in this field.[1] As it is, it is not clear why Trachtenberg privileges some books over others. There is, for example, no mention of Robert Butow's *The John Doe Associates: Backdoor Diplomacy for Peace, 1941* (Stanford: Stanford University Press, 1974) or of the essays in Hilary Conroy and Harry Wray (eds), *Pearl Harbor Reexamined: Prologue to the Pacific War* (Honolulu: University of Hawaii Press, 1990). In addition, it seems odd that he brings primary sources into this case study, for it precedes the chapter on working with documents. Would it not have been better to have placed the case study at the end of the book so that students could see how all the threads of research can be drawn together?

In terms of his analysis the first thing to note is that Trachtenberg, in choosing to look at American entry into the Second World War, has not picked a simple example. One difficulty is that the Roosevelt administration's policy-making process was notoriously opaque, which means that it is far from easy to discern the president's thoughts and motivations. It is this, as much as anything else, which has allowed conspiracy theorists to have a field day with this topic. In addition, another problem is that any attempt at analysis has to take place in the shadow of one of the last century's most famous events, the Japanese surprise attack on Pearl Harbor on 7 December 1941. The sheer drama of this assault means that when studying this period it is hard to break free from one of the great difficulties posed by any analysis of war origins, namely how to avoid a deterministic approach that treats the conflict as inevitable and the eventual combatants as fully cognizant of the consequences of their actions.

Determinism's allure is represented in Trachtenberg's study by his attraction to the 'back door to war' thesis, which seeks to explain American policy towards Japan by suggesting that Roosevelt saw events in the Pacific as a roundabout means of involving the United States in a conflict with Germany. According to this idea, Roosevelt followed a tough policy towards Japan in the hope and, indeed, the expectation that this would precipitate a general war against the Tripartite Powers. On weighing the evidence, Trachtenberg implies that this is a more convincing explanation than the idea that American policy in the Pacific was designed to contain and deter Japan, which is the argument most often advanced by detailed studies of this topic.

One of Trachtenberg's objections to the containment thesis relates to the inflexible nature of the American oil embargo, 'which he [Roosevelt] knew would in all probability lead to war with Japan' (100). To Trachtenberg this inflexibility can be interpreted as angling for war. However, the argument that this was Roosevelt's intention is open to doubt. Waldo Heinrichs, in the most authoritative account of American policy in 1941, argues that Roosevelt's oil embargo was designed to achieve 'the immobilization of Japan' and makes it clear that this ran in parallel with a determined effort to strengthen U.S. forces in the

[1] Michael Barnhart, 'The Origins of the Second World War in Asia and the Pacific: Synthesis Impossible?' in Michael J. Hogan (ed.), *Paths to Power: The Historiography of American Foreign Relations to 1941* (Cambridge: Cambridge University Press, 2000) 268-295.

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Philippines.[2] In other words, Roosevelt favoured tightening the screws on Japan not because he wished to precipitate hostilities but because he believed that the Japanese could be deterred. This was, of course, a grave error of judgement, but it points to one of the main problems in 1941: the United States, due to its underestimation of Japan's military capabilities, followed a deterrence policy without actually having a sufficiently effective deterrent in place.

Washington's policy, of course, contained within it the risk of war, but even then one has to ask whether policy-makers in Washington believed that the failure of containment would necessarily lead to a direct and immediate American-Japanese conflict or whether the road to hostilities would be more circuitous. In other words, was there a clear-cut 'back door to war'? The evidence cited by Heinrichs suggests that there was a great deal of uncertainty about Japan's future policy even up until the outbreak of hostilities with many believing that its next move would be against Thailand. It was only on 5 December that it became clear that Malaya was in its sights and no firm evidence existed prior to the outbreak of war that there would be a direct assault on American territory.[3] This confusion hardly suggests that Roosevelt was a puppet master controlling the situation, for there was no guarantee that Congress would declare war on Japan if the latter attacked Thailand or even perhaps the British or Dutch colonial territories. In the end Roosevelt was let off the hook and did not have to rally Congress into declaring war over an indirect threat to American security, but this could not have been predicted.[4]

Trachtenberg also suggests that Washington's tough stance towards the Hull-Nomura talks is an indication of ulterior motives. He observes that if the United States only wanted to contain Japan then it need not have pursued a hard line over China in these talks, which was tantamount to pushing for a Japanese retreat. In making this argument, however, he fails to address two key issues that exercised policy-makers in 1941. The first is that he overlooks the fear that to take anything less than a tough line on China might have a catastrophic effect on Chinese morale leading to a collapse that would have freed the Japanese army to strike against British, Dutch and Russian territory.[5] The second problem is that the administration needed to avoid any perceived selling out of Chinese interests for

[2] Waldo Heinrichs, *Threshold of War: Franklin D. Roosevelt and American Entry into World War II* (New York: Oxford University Press, 1988) 179.

[3] Heinrichs, *Threshold of War*, 215-216. Unfortunately on one of the few occasions when Trachtenberg does mention intelligence, he resurrects the unsubstantiated claim made by James Rusbridger that the British government has refused to release Churchill's supposed 'second telegram' to Roosevelt of 26 November 1941. There is no evidence that any such telegram ever existed.

[4] Robert Dallek, *Franklin D. Roosevelt and American Foreign Policy, 1932-1945* (New York: Oxford University Press, 1979) 307; David Kaiser, 'Conspiracy or Cock-up? Pearl Harbor Revisited', *Intelligence and National Security*, vol. 9, no. 2, 1994, 357-358.

[5] Jonathan Utley, *Going to War with Japan 1937-1941* (New York: Fordham University Press, 2005 edn) 159, 165, 175.

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this might raise the prospect of it being accused of appeasement, which was by then probably the dirtiest word in the English language.[6] In other words, Trachtenberg assumes that the Roosevelt administration had room to manoeuvre in regard to the China issue, when in fact military and political reasons dictated otherwise. Nor does Trachtenberg take on board the role of human frailties, such as the damage done by Hull's fatalism and lack of strategic wisdom or Nomura's naïve and misleading diplomacy.

Trachtenberg's treatment of Japan suffers from similar failings, for by drawing on the work of Tsunoda Jun to argue that the Japanese were willing to compromise to avoid war, he overlooks the evidence which suggests that Tokyo's thinking was considerably less flexible. It is worth noting here that in a footnote he observes that that it is 'unusual' that the 'translator' of Tsunoda's work, David Titus, wrote a critical introduction when it was published in English (107). In fact, the criticisms that Titus, who is an historian in his own right, made are well-founded and deserving of Trachtenberg's attention, for they explain the subjective approach that Tsunoda took towards this topic and the flaws in his analysis.[7] In addition, Trachtenberg cites the American and British ambassadors in Tokyo, Joseph Grew and Sir Robert Craigie, to support his argument that Japan was prepared to negotiate, but does not acknowledge that their advice was disregarded because they had been wrong about Japanese moderation so many times before.

The situation in 1941 was thus more complex than Trachtenberg imagines. The Roosevelt administration was buffeted by a variety of factors over which it had little control and its decision-making was, due to its misperception of Japanese capabilities, adversely affected by its inability to understand how Japan was likely to react. Japan meanwhile may have been torn between war and peace, but the 'hawks' were in the ascendancy and, moreover, the short-term military balance favoured a Japanese attack. Thus, while the 'back door to war' interpretation might offer an attractive conceptualization of American policy and fits in with a realist approach that sees all actions as rational and calculated, it can only work as an explanation if one downplays the role of personality, miscalculation, and the influence of domestic politics. In the end, Trachtenberg's analysis provides an over-simplified, deterministic picture of a very complex historical event. In this lies a lesson that should be part of his overall guidance to students, which is to deal with the temptation and dangers of revisionism. When one finds oneself differing with the general consensus about an historical event it is wise to pause and to reflect on why one's own conclusion is out of line with that of most other historians.

[6] See Richard J. Grace, 'Whitehall and the Ghost of Appeasement: November 1941', *Diplomatic History*, vol. 3, no. 2, 1979, 173-191.

[7] David Titus, 'Introduction', in James W. Morley (ed.), *The Final Confrontation: Japan's Negotiations with the United States, 1941* (New York: Columbia University Press, 1994) xix-xxxviii. For a recent well-balanced account of Japan's road to war, see Noriko Kawamura, 'Emperor Hirohito and Japan's Decision to Go to War with the United States: Reexamined', *Diplomatic History*, vol. 31, no. 1, 2007, 51-79.

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Review by John Ferris, University of Calgary

This is a solid work by any standard. It starts with a brief but judicious assessment of some foundations for the study of international history, such as the nature of causation and interpretation and recent theories of history and International Relations (IR). Then, through several case studies, Trachtenberg demonstrates how to parse relevant secondary works, how to criticize the interpretations of others and to offer your own. These examples demonstrate how research is guided by intuition, memory, logic, and serendipity, and illustrate how evidence and rhetoric are combined to make cases or to kill them. Finally, Trachtenberg discusses how to define a viable research topic, locate secondary literature, find primary documents in several countries, and turn evidence into text. Any graduate student working in international history can profit from this book: I wish something similar had been available when I started in strategic history. Yet if the work thus achieves its aims, it still remains open to criticism.

John Ferris *is a Professor of History at The University of Calgary, where he also is a Fellow at The Centre for Military and Strategic Studies. He received a PhD (1986) in War Studies, from King's College, The University of London, United Kingdom. He has published four books and sixty academic articles or chapters in books, on diplomatic, intelligence and military history, as well as contemporary strategy and intelligence. His books have been published in Australia, Canada, France, Japan, Singapore, Turkey, The United States, and the United Kingdom: they also have been translated into French, Hebrew and Japanese. He comments in national and international media, on Canadian and American foreign and military policy, the wars in Iraq and Afghanistan, intelligence, and nuclear weapons.*

Trachtenberg does not define the field he describes. He ignores any form of postmodern IR or history. True, many of these schools do not do international history, as Trachtenberg (or I) would understand it, let alone do what it does, only better. Instead, they address other topics and name them international history, often with the aim of delegitimizing its older forms. Yet students of international history might encounter these bodies of thought, and they must learn of others which do affect our literature. The latter include the neo-realist, liberal, and constructivist schools of IR; work inspired by the 'linguistic turn' view that language or ideas order our comprehension of reality; and the concepts about the relationship between evidence, perceptions, and decisions which are embraced by contemporary scholars of intelligence. Trachtenberg does address some of these matters, but less systematically than is necessary for his audience.

Historians are not innocent of ideas, just naïve about them. Students will gain from knowing which concepts they have at their disposal, and which ones possess them. They will profit from understanding that power can be defined variously as current military capacity (John Mearsheimer), as relative economic strength in the long term (Paul Kennedy), or as the outcome, often mystified, of struggles for personal, political and social dominance within states (Michel Foucault, Antonio Gramsci or E. P. Thompson), and that it may be measured in different ways. Most international historians today combine liberal

and realist views, and some could be called liberal constructivist realists; students would do well to know the meaning of these elemental ideas about international relations, and of their alloys.

Moreover, Trachtenberg implicitly defines international history as being diplomatic history, without discussing what that is or how else the subject might be understood. Diplomatic history, with its focus on the relations of senior decision makers within and between states, is a proud profession. The study of politics, diplomacy, and strategy are the oldest forms of history, first mastered by Thucydides and central to the modern discipline which emerged two centuries ago. But after 1960, diplomatic history began a decline. Though a grim band of veterans, led by giants such as Paul Schroeder and Zara Steiner, stood to inspire newcomers, the discipline became marginalized, vulnerable to accusations of focusing on superficial rather than substantial matters or of embodying the Dead White European Male in academe. Once a multi-archival, multi-national, and multi-linguistic enterprise, it shrank into schools of national diplomatic history, haunted by a collapse in the comprehension of languages and the sometimes sectarian and parochial nature of the study of American foreign relations.

Yet in the last decade diplomatic history has perceptibly recovered and the field has also been enriched in other ways. One is through the study of the international relations of non-state actors, whether intellectuals, churches, or mass media; another is through work on the interplay between power, culture, sentiment, and politics. Yet another stems from the renaissance in military history: strategic history now fuses military and international history, assessing such things as military policy in peacetime and diplomacy in war, what power is and what it can and cannot do.[1] Yet another contribution comes from the sub-field of intelligence, with its distinctive approach to evidence, perceptions and ideas as means to understand how and why actions are taken. International history equals diplomatic history, transformed by interaction with new provinces such as these.

The import of these observations can be shown by applying them to Trachtenberg's main case study on the origins of the Pacific War, and by criticizing his argument according to his own prescribed methods. Through his close reading of secondary works, Trachtenberg rejects the standard view that Japanese aims were belligerent and American policy defensive. Instead, applying the central insight of strategic studies that the distribution of power and the structure of systems drive the actions of polities seeking to further their security, he argues that the United States practised power politics like any other nation; moreover, rather than trying just to defend weak and endangered countries or to deter Tokyo from aggression, Washington drove a reluctant Japan to war—and it did so intentionally. In fact, Trachtenberg ascribes to the United States a greater and more aggressive role in causing war in 1941 than Germany had in 1914, according to his critique of the work of Fritz Fischer (67-73).

[1] For further discussion, see John Ferris, 'Power, Strategy, Armed Forces and War', in Patrick Finney (ed), *Palgrave Advances in International History* (London: Palgrave, 2005) 58-79.

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Trachtenberg rightly distinguishes his argument that Washington drove Tokyo to war from the conspiracy theory that Franklin Roosevelt knowingly let Japan attack Pearl Harbor. Instead, he claims that had Roosevelt aimed simply at deterrence or containment, 'Japan was clearly willing to be contained' (115). But he had greater ambitions: rollback. Roosevelt believed that a total oil embargo would drive Japan to attack the Netherlands East Indies (NEI), which would necessarily create a Pacific war rather than a localized one, and so, with the application of that embargo, he 'deliberately opted for a policy which he knew would in all probability lead to war with Japan' (100). American authorities also rejected an effort by the Japanese Prime Minister Prince Konoye to solve mutual differences through a meeting with Roosevelt—which might have succeeded since Konoye was 'searching desperately' for a way to avoid war (110)—and a later Japanese proposal for a modus vivendi, involving a withdrawal of their troops from Indochina and pursuit of a negotiated peace in China. Roosevelt took these steps largely so that Japan would attack the United States and provide a pretext for war with Nazi Germany, which he regarded (correctly) as a deadly threat to American security that must immediately be addressed.

This case rests on Trachtenberg's reading of the literature about the outbreak of the Pacific War, which is somewhat problematic. The most striking characteristic of this literature is its division into self-contained fields. Some of these divisions are thematic (military, diplomatic, etc.) but the greatest are those between different national international histories. Given the barriers of language, it is perhaps not surprising that the Japanese literature is distinct, but writing on the United States and the United Kingdom also often stands in isolation, as does that on the Netherlands, the Dominions, the USSR, and China. Trachtenberg understands the American diplomatic history literature, but not the British one, where he uses Christopher Thorne's magisterial *Allies of a Kind: The United States, Britain, and the War Against Japan, 1941-1945* but overlooks more recent work by Antony Best or the Anglo Japanese History Project.[2] His bibliography on Japanese decisions is small, but if it includes most major works published in English it excludes a controversial but serious polemic which challenges his position.[3] Finally, the discussion of the specialist literature on intelligence is inadequate. Trachtenberg ignores the intelligence failure at Pearl Harbor and the standard works on the topic—even though he must know them—which offer the best analyses of the preconceptions about Japan which shaped American policy.[4] He misses two excellent monographs on British intelligence and Japan, while

[2] Christopher Thorne, *Allies of a Kind: The United States, Britain and the War Against Japan, 1941-1945* (London: Hamilton, 1978); Antony Best, *Britain, Japan and Pearl Harbor: Avoiding War in East Asia, 1936-41* (London: Routledge, 1995); Ian Nish and Yoichi Kibata (eds), *The History of Anglo-Japanese Relations, 1600-2000. Volume Two: The Political-Diplomatic Dimension, 1931-2000* (London: Macmillan, 2000).

[3] Herbert Bix, *Hirohito and the Making of Modern Japan* (New York: HarperCollins, 2000).

[4] Roberta Wohlstetter, *Pearl Harbor: Warning and Decision* (Stanford: Stanford University Press, 1962); Gordon W. Prange, with Donald M. Goldstein and Katherine V. Dillon, *At Dawn We Slept: The Untold Story of Pearl Harbor* (New York: McGraw Hill, 1981) and *Pearl Harbor: The Verdict of History* (New York: McGraw Hill, 1986); Henry Claussen and Bruce Lee, *Pearl Harbor: Final Judgement* (New York: Crown, 1992).

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directing students toward a discredited piece of Pearl Harbor conspiracy theory.[5] He also seems unaware that since 1995 the United States and United Kingdom have opened almost all previously closed intelligence files on the events of 1941. The intelligence record is no longer unknown, even if it is often still overlooked.

In developing his interpretation of responsibility for the Pacific War, Trachtenberg thus follows a spotty map across rough terrain. Moreover, to assess the precise causal significance of any actor involved in a complex event is hard, because all influence the outcome—even someone who struggles against a particular outcome can be held responsible for doing so insufficiently well. Thus Luigi Albertini blamed Britain for shaping the outbreak of the Great War because of what it did not do but should have done during the July crisis. The issue is also complicated because historians define causation in conflicting ways. Should only new developments which change old patterns be classed as causes? Should one be held responsible only for outcomes which one intends, or for whatever effects flow from one's actions? Or for any outcomes which one should have known were possible or probable? If one reacts in a predictable, normal, manner to the behaviour of another, did they cause your actions or are you responsible for what you did?[6] Each definition has some force and the trick is to determine which is applicable when. In multilateral relationships, intention and effect become even more tangled. Some events occur by design, but more occur by mistake or through systems failures. Effects occur which no one had wished. In a game with five players, a good stroke against one rival may alarm a third and lead them to bump a fourth into deflecting one's shot—perhaps towards one's own net. Confusion over such issues hampers Trachtenberg's case.

Trachtenberg's assessment of Japanese policy is skewed. He is right to argue that Japanese decisions were affected by those of other countries, and might have changed had American policy done so. Still, it does seem odd that after criticizing A. J. P. Taylor for arguing that Adolf Hitler did not really cause German actions, Trachtenberg should then suggest the Japanese did not start the Pacific War—the devil made them do it. Of course, American diplomatic historians often assume that the United States must have governed any bilateral relationship in which it was involved, which produces odd views such as the old New Left one that the USSR was not really responsible for its own actions in the early Cold War. But during 1941 Washington did not determine Japanese actions; it merely shaped them, as Tokyo shaped American behaviour in turn. Japanese decisions were made in Tokyo where, the specialist literature generally concludes, most decision makers were belligerent and committed to—or unwilling to abandon - a policy they knew would likely cause a war with

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[5] Richard Aldrich, *Intelligence and the War against Japan: Britain, America and the Politics of Secret Service* (Cambridge: Cambridge University Press, 2000); Antony Best, *British Intelligence and the Japanese Challenge in Asia, 1914-1941* (London: Palgrave, 2002); James Rusbridger and Eric Nave, *Betrayal at Pearl Harbor: How Churchill Lured Roosevelt into World War II* (New York: Summit Books 1991).

[6] The classic assessment of these issues is W. H. Dray, 'Concepts of Causation in A. J. P. Taylor's Account of The Origins of the Second World War', *History and Theory*, vol. 17, no. 2, 1978, 149-74.

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some or all of the western powers.

Trachtenberg tries to discredit this dominant view through sleight of hand. He claims that it hinges on crude assumptions about military predominance in Japanese politics, hits a caricature of it and leaves the unwary to imagine it is dispatched when it is in fact barely scratched. Then he directs students from that literature toward a minority one, consisting essentially of four writers whose views he does not criticize. Two of them, the British and American ambassadors in Tokyo during 1941, Sir Robert Craigie and Joseph Grew, understood much about Japan and saw where it was heading but also routinely overestimated the power of 'liberal' elements. Hence they must be treated with caution as witnesses, as most historians have done for decades. The third source is a fifty year old book by Paul Schroeder, a pioneering effort based on western documents and evidence from the International Military Tribunal at Tokyo. Oddly, Trachtenberg rejects Schroeder's authority on the issue he knew best—American policy—but accepts it on that he understood least well—Japanese policy. Tsunoda Jun, the last of these sources, is an authority on Japanese policy, but one whose views are at odds with the dominant opinion.

In this context, the answer to the question of whether the Konoye proposal or that for a modus vivendi could have facilitated a rapprochement must be: perhaps, but not so easily as Trachtenberg assumes. The United States refused these proposals partly because to accept them would have alarmed both China and Britain, states Washington needed to keep firm so they would hold Japan in check. The modus vivendi could easily have undermined Chiang Kai-shek's position, which is why Britain opposed it; and given everything they knew, it does seem hard to blame Winston Churchill and Roosevelt for refusing to make concessions to a belligerent Japan and to assist its unpleasant occupation of China. In any case, as happened with Admiral Kurusu's talks in Washington, such negotiations might simply have been used as cover for a Japanese attack. Students who peruse the literature as a whole may reasonably conclude that Japan was more likely to go to war than Trachtenberg suggests and less potentially susceptible to a reasoned American policy, which reduces the significance of Roosevelt's intentions as a cause for Japanese actions.

Nor are Roosevelt's intentions easy to assess. Trachtenberg assumes that outcomes emerged as a result of willed aims and conscious action, and therefore can be inferred from the workings of the American—and even the British—machine of state. In fact, these machines functioned in a chaotic fashion during 1940-41 because the two states suddenly confronted unprecedented problems of strategic policy, which drew many organisations and agencies into new relationships at home and abroad. Britain and the United States became allies without an alliance. Their organisations, shacked up in an intimate and illicit embrace, tripped over each other; moreover, Churchill and Roosevelt assumed control from their bureaucracies on the most central issues and dealt with them within their personal bilateral relationship. At the same time, Japan was overall regarded as a secondary matter, with policy consequently often left on autopilot. Trachtenberg takes decision making for granted, describing this confusion but using evidence drawn from it to depict a rational actor executing his intentions. Yet this confusion distorted rational

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decisions, and the relationship of intention and effect.

Trachtenberg also treats all statesmen as 'diplomatic man'—rational actors who understood power and practised power politics in the same ways, in line with the dictates of realism. Though he accepts that non-realist thinking can affect statesmen, he dislikes the notion and seeks to minimize its significance, often in odd ways. Thus he attacks the argument that American leaders could ever have been affected by sentiments of liberalism, legalism, or morality by conflating it with the larger idea 'that policy makers were prisoners of America's liberal ideology, that they had to stand up for their moral beliefs, and that they were incapable of conducting a policy based on realist principles' (122), and then by identifying occasions when American leaders thought as realists. This argument hits a straw man and misses its real target. Critics do not claim that American decisions were entirely liberal and never realist, rather that they were shaped by both currents, and too much by liberalism. This argument cannot be refuted simply by locating random instances of realist thinking, but only by assessing the latter's interaction with ideology.

International historians characteristically find difficulties with this matter. They posit a conflict between 'realism' and 'idealism', even though realism is a form of idealism if ever there was one. When they say 'idealism' international historians usually mean 'liberalism', but that is merely one of many ideologies. Nor is ideology a dirty word. It is a fact of statesmanship. Ideology conditions our means to know the world and hence to shape it, our views of power, interest, force, of the game and its rules. Realism is essential to the understanding and the playing of power politics, and the ways the former shapes the latter. But it tells you how to play games, not which ones or why. Realism describes power and means, but it cannot define values or the ends they mandate. A pure realist, aiming at power and victory for their own sake alone, is a psychopath. Few statesmen are—none in the period before the Pacific War. Almost all statesmen merge realism with some form of values which join means to ends. The realism of real statesmen combines ideology and an understanding of instrumentality. By removing ideology from this alloy analysts do not derive a purer form of realism, they destroy it. They reduce statesmanship to gamesmanship, just as removing copper from the smelt leaves not better bronze, but tin. Realism is a noun in search of a qualifying adjective, whether 'Marxist-Leninist', 'national socialist' or 'liberal'.[7]

These difficulties, and a failure to treat perceptions as problematic, haunt Trachtenberg's interpretation, especially of Japanese policy. He denies that Japanese leaders could have wished to fight the United States, because they should have known they were outweighed and must lose:

> When you look at that view of an uncontainable and undeterrable Japan, part of you says, 'it just can't be, states just don't behave that way'—a reaction that shows that a certain theory of international

[7] For further discussion, see John Ferris, 'Image and Accident: Intelligence and the Origins of the Second World War, 1933-1941', in John Ferris, *Intelligence and Strategy: Selected Essays* (London: Routledge, 2005) 99-137.

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> politics has come into play. Everyone knew that in terms of mobilizable war potential, the United States was a vastly stronger country than Japan. The figures for steel production, for example, were common knowledge at the time. So why, you say to yourself, would a country like Japan, already bogged down in a war with China, insist on pursuing a policy of expansion knowing full well that there was a very good chance that it would lead directly to war with America? It's hard to believe that the Japanese government—or any government, for that matter—would behave that way, no matter what a series of distinguished scholars say. Of course, you are not certain on those general grounds that that view of an uncontainable Japan is wrong. There is no guarantee that countries will behave rationally. But your sense for how international politics works makes you reluctant to accept that argument on faith. (103-104)

If the Japanese had really thought in this way, or only in this way, they would never have gone to war at all; but they did. Nor were they alone in making such errors. The influence of ideology and perception meant that during 1938-41 the leaders of every major state constantly interpreted power in different ways, failed to effect their intentions, and made mistakes on fundamental issues. Instead of realists behaving rationally, the record shows statesmen behaving badly. They did not act as we would expect by the dictates of our fusion of realism, liberalism, and materialism. Instead, culture and ideology entered their equations. We cannot understand how these statesmen acted without considering how they thought.

Trachtenberg also tries to explain Roosevelt's policy without examining his preconceptions or those of his subordinates. By 1941, American decision makers were irritated with Japan and eager to rein it in. They regarded Japanese leaders as rational and cautious—unlikely to start a war they must know they would lose—and their armed forces as formidable close to home but unable to strike far away. Until 7 December 1941 no one imagined Japan could attack as far as it did, or as hard, or in so many places at once. For most of 1941 Roosevelt and his naval, military and diplomatic officials doubted that Japan would dare fight the United States, as opposed to attack the NEI—which, pace Trachtenberg, need not have meant war with the United States or even Britain. Nor did Roosevelt necessarily have just one aim—statesmen often act so as to create a range of options from which they can select in an opportunistic fashion. Roosevelt probably moved with many aims in mind. He pressed Japan because he was tired of its belligerence and wanted to send it a signal, which he thought could be done without much risk of war. He pursued deterrence, containment, and rollback all at once, because he was not sure which would work best, and differentiated less clearly between them than we do.

Roosevelt thought Japan would back down rather than attack, because he overestimated his bargaining position and underestimated both Japan's ability to strike out of area and its recklessness. (The biggest intelligence failure of 7 December 1941 occurred in Tokyo, not Washington.) Mistakes and systems failure shaped the outbreak of the Pacific War as much as calculation and the intentions which most mattered were those of Tokyo. This argument saps Trachtenberg's line, though not entirely. Roosevelt must always have known that his actions might make Japan attack the United States, and he was willing to accept that risk, or opportunity. The possibility seemed increasingly likely in the weeks before 7 December, doubly so when Britain and the United States rejected the modus vivendi, but such an

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attack was expected to occur in the spring of 1942 when great reinforcements would have bolstered British and American power in the theatre and the target of December 1941 was expected to be Thailand.

Meanwhile, Roosevelt's subordinates and the British jostled his elbow, but not quite in the manner Trachtenberg suggests. The British would have liked to entangle the United States in an alliance, but knew this could not be done directly. They tried to avoid appearing to manipulate Washington, but did so by following its line, going not one step further than it did. Whitehall was also driven toward a hard line because it needed to buy time to check an immediate threat, to keep Chiang Kai-shek in office and China fighting Japan, and to overcome the impact on Washington of the image of appeasement. These imperatives did buffet American policy, but Britain's real significance was not as subject but object. The United States sought to sustain Britain until Nazi Germany was defeated, whereas Japan wished to devour a rich, weak, and malevolent neighbor, to exploit an opportunity which comes once in a thousand years while the world seemed ready to turn upside down. Japan underestimated American and overrated German power. Its leaders expected Germany to transform the world and contain the United States, just as American policy makers feared it might; this shared assessment spurred both states toward the Pacific War. Both sides made mistakes, but the Japanese made more.

The thrust of these observations is to support Trachtenberg's mode of analysis, but only part of his argument. This is a story without heroes or villains, a world of states using strength to pursue interests, acting on and driven by their reading of power, caught in a tangled web of causation where nothing worked as anyone intended and paradox was king. Japan attacked so as to preempt a threat it had provoked. Like German decision makers in 1914, American ones in 1941 acted in ways they knew might force another side into war, but with a greater degree of confusion. Roosevelt, expecting to coerce a weaker state, instead pushed Japan toward war. He did so by combining present weakness and hostility with a looming and potentially fatal menace. This effect was not quite what he planned, but he could live with the outcome because it realized a greater aim: to have Germany bring the United States into a European war. The effect he did not intend let him achieve the intention he could not otherwise effect.

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Review by Petra Goedde, Temple University

Historians charged with the task of teaching a research seminar in international history should greet the appearance of Marc Trachtenberg's *The Craft of International History: A Guide to Method* with enthusiasm. The book, as the author states in his first sentence, 'is a practical guide to the historical study of international politics—a guide to how historical work in this area can actually be done, a guide which people working in this field might actually find useful' (vii). As a 'nuts and bolts' manual to research the book is a valuable tool. Trachtenberg's detailed advice concerning the mastery of secondary literature is exemplary. Readers learn to identify the component parts of a good history book, ranging from major thesis to sub-theses to the structure of the argument. They learn ways to examine the soundness of documentary evidence. Most importantly they learn that reading is not a passive exercise of turning page after page, but an active engagement with an argument. This undoubtedly is the first step toward developing meaningful historical questions and an independent research strategy.

Petra Goedde *received her Ph.D. from Northwestern University. She is an Associate Professor in the Department of History at Temple University. She is the author of several articles and the book* GIs and Germans: Culture, Gender, and Foreign Relations, 1945-1949 *(New Haven: Yale University Press, 2003) and is currently at work on a book-length project on the global discourse on peace during the early cold war, as well as a chapter on global culture since 1945 for a multi-volume World History, edited by Akira Iriye and Jürgen Osterhammel. In addition she is co-editing with Richard Immerman* the Oxford Handbook on the Cold War. *She is currently serving as chair of the 2007-2008 Bernath Book Prize committee of the Society for Historians of American Foreign Relations (SHAFR).*

Perhaps the most practical part of the practical guide is the appendix, which lists an impressive array of print and web sources, a veritable treasure chest for historians of international relations as well as political scientists. Most professors of international history will certainly be well advised to direct their graduate students to these pages—in fact most scholars in the field will probably return to this section frequently themselves as a starting point for their own research, though Trachtenberg's updated website might be ultimately more valuable than the appendix to the book.

Yet as a blueprint for research projects in international history, the book does not reflect the current state of the field. Rather it serves two primary constituents: students of politics who are interested in pursuing historical questions, and historians of foreign relations who are interested in high politics. International history, however, has expanded dramatically in the last decade and a half beyond the area of political and diplomatic history. The field now includes area specialists as well as cultural, social, economic, and borderlands historians.[1]

[1] For a good introduction to the state of the field, see Patrick Finney (ed.) *Palgrave Advances in International History* (London: Palgrave, 2005).

Trachtenberg makes a gesture of inclusion toward such new approaches to international history. He argues—and nearly all historians would agree—that theory and methodology are vital components of historical work, that any historical work 'has to have a strong conceptual core' and that 'evidence needs to be approached with specific questions in mind' (vii). Trachtenberg might have made a stronger case for the importance of conceptualization. Stories of the past develop meaning through historians' work of conceptualization. Historians are constantly involved in the process of assessing and re-assessing primary evidence based on shifting political, social, cultural, and economic contexts. That does not mean that every work of history is merely a reflection of a particular historian's political or cultural preferences or of the political climate of the times. Instead historians constantly build on previous work to construct an ever more intricate and complex picture of the past. This is true for international history as for any other field within the historical profession.

The book only briefly discusses historical theories, beginning with Carl Hempel and R.G. Collingwood, even though Trachtenberg himself acknowledges that neither scholar's 'school' was taken particularly seriously (6). Yet with the help of these two theoreticians Trachtenberg guides the debate toward the question in which he is most interested: can historians reconstruct an objective, knowable, and truthful past? Can scholars write history, as Leopold von Ranke famously put it, 'wie es eigentlich gewesen' (as it really was)? It is quite clear where Trachtenberg's sympathies lie. He dismisses the likes of Hayden White and the postmodernists who followed him, who argued that the construction of a coherent historical narrative rests at least in part on imagination. These scholars lent, he charged, 'a veneer of philosophical respectability to the new antiobjectivist view of history' (12). Trachtenberg instead advocates philosophy of science as a better guide to historical methodology. While he acknowledges that 'the idea that the aim is to learn the truth about the object of study can safely be abandoned,' he nonetheless wants historians to make 'the past intelligible' through exploring the historical data in an objective, scientific way (23).

Trachtenberg's selective and at times dismissive treatment of historical theories is the book's greatest weakness. He often writes about 'theory' in the singular, as if the only theory scholars of international history have to concern themselves with is International Relations (IR) theory (37). This might do for some political scientists—although they, too, might take issue with this approach—but it won't for historians. International history is a burgeoning field on the cutting edge of historical scholarship, yet after reading Trachtenberg you would not get that sense. His narrow conceptualization of international history no longer reflects the current state of scholarship. The field's practitioners have offered creative ways of engaging with historical materialism, postmodernism, world systems theory, culture, race, and gender.[2] By engaging with those bodies of literature

[2] See, for instance, Fernand Braudel, *The Mediterranean and the Mediterranean World in the Age of Philip II* (New York: Harper and Row, 1972); William H. McNeill, *The Rise of the West: A History of the Human Community* (Chicago: University of Chicago Press, 1963); Joan Wallach Scott, G*ender and the Politics of History*

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students of international history in recent years have managed to reconnect to other historical fields and begun a lively and stimulating exchange that has given the field a significant boost.[3]

In place of interpretive theory Trachtenberg offers a fact-and-documents-based positivism. The book could be read as a spirited defense of what Mary Fulbrook has labeled 'source fetishism' and 'archive positivism'.[4] Historical evidence, according to the appendix, is to be found either in published versions of archived material, policy papers of leading statesmen, or in the archives themselves. Only when dealing with recent events does Trachtenberg recommend relying on what he calls 'open sources: on newspaper and magazine accounts, on statements made by government officials, on testimony in congressional hearings, and the like' (249). Yet some of the most interesting recent work in international history has relied on non-archival sources: popular magazines, newspapers, film, novels, and non-fictional monographs. Such work presents a conceptualization of international history beyond the realm of high politics. For Trachtenberg, however, international history primarily consists of striking a balance between only two concepts, power and statecraft, the stuff of elite politics: 'Power realities are of fundamental importance,' he states, 'but statecraft also has a major impact on the course of events, and in doing history both invariably have to be taken into account' (67). This approach to doing history is more reminiscent of Ranke's *Primat der Aussenpolitik* than anything produced since then.

Two words surface repeatedly throughout Trachtenberg's text: 'basic' and 'real.' They are indicative of the author's faith in an objective and knowable past. Not only does Trachtenberg admonish students to search for the 'real' and focus on the 'basic' (for example, Hitler's 'basic goals' (65) or 'the real historical problem' (92)), he also writes with great assertiveness (and less evidence) about those 'real' and 'basic' historical elements. Yet can historians actually know what Hitler's 'basic goals' were? An historian's notion of these basic goals is at least in part the result of precisely the conceptualization

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(New York: Columbia University Press, 1999 edn); Immanuel Wallerstein, *The Modern World System,* 3 vols (New York: Academic Press, 1974-1988); Immanuel Wallerstein, *World-Systems Analysis: An Introduction* (Durham: Duke University Press, 2004).

[3] Tim Borstelmann, *The Cold War and the Color Line: American Race Relations in the Global Arena* (Cambridge, MA: Harvard University Press, 2001); Mary Dudziak, *Cold War Civil Rights: Race and the Image of American Democracy* (Princeton: Princeton University Press, 2000); Christopher Endy, *Cold War Holidays: American Tourism in France* (Chapel Hill: University of North Carolina Press, 2004); Penny von Eschen, *Race Against Empire: Black Americans and Anticolonialism, 1937-1957* (Ithaca: Cornell University Press, 1997); Petra Goedde *GIs and Germans: Culture, Gender and Foreign Relations, 1945-1949* (New Haven: Yale University Press, 2003); Greg Grandin, *Empire's Workshop: Latin America, the United States, and the Rise of the New Imperialism* (New York: Metropolitan, 2006); Barbara Keys, *Globalizing Sport: National Rivalry and International Community in the 1930s* (Cambridge, MA: Harvard University Press, 2006); Christina Klein, *Cold War Orientalism: Asia in the Middlebrow Imagination, 1945-1961* (Berkeley: University of California Press, 2003); Naoko Shibusawa, *America's Geisha Ally: Reimagining the Japanese Enemy* (Cambridge, MA: Harvard University Press, 2006).

[4] Mary Fulbrook, *Historical Theory* (London: Routledge, 2002) 3.

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Trachtenberg advocates at the outset. Because there are different ways of conceptualizing the historical past, and because conceptualization depends on the theoretical approach each historian takes, historians produce multiple and at times contradictory interpretations of the past.

While few historians would want to side with those who claim all history is a mere construct, most would also argue that there is no single way to write the history of the Second World War, Trachtenberg's main case study. It all depends, as Trachtenberg rightly points out, on the kinds of questions one asks. For all of Trachtenberg's emphasis on documents and archives, the historian's work does not end with identifying documents, events, and dates. It begins with it. It is through a particular conceptualization, through analysis and interpretation that researchers make sense of the historical past. And this work creates not one but several, at times competing, historical 'truths.' Trachtenberg's frequent use of the terms 'real' and 'basic' imparts the impression that there are real and basic historical questions as opposed to false and superfluous ones (including presumably, questions about race, gender, popular culture, and the like); that there is one right way of doing history.

This returns us to the issue of power and statecraft and the relevance of culture and historical theory. Not all answers to questions of war, peace, power, and politics lie in the state archives of the great nations. The theoretical perspectives that Trachtenberg ignores have in many cases contributed to a better understanding of how states and cultures interact. Because of these new approaches scholars rarely speak of nations in monolithic terms (although Trachtenberg does on occasion). Most also try to avoid formulations like 'states just don't behave that way' (103), because states don't behave, people do. International historians recognize that within each state competing factions are often driving for one political objective or another, each with its very own agenda. That agenda might be driven by politics but also by economic considerations, cultural assumptions, prejudices, and misperceptions. Cultural historians of foreign relations are interested in exploring those assumptions that underlie a particular political position, and in doing so they do not ignore questions of power. On the contrary, they are acutely aware of how power differences both among nations and within a given state are expressed and legitimized through cultural references. Economic historians and world systems theorists focus on the larger economic structures underlying the unequal distribution of power in the world.

Trachtenberg's research guide provides no suggestions for the exploration of these cultural and economic questions, and thus he will take students only in one direction—the direction of high politics. Yet international history encompasses a far broader spectrum of scholarship, much of it now done in fields outside of diplomatic history. If diplomatic historians want to remain engaged in these debates in international history, they would do well to extend their agenda beyond what Trachtenberg offers here.

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This is not to say that the advice he offers is of little value—there is just a lot more out there that students should know. As the field of international history continues to expand and redefine itself, trying to define the nature of research in it might be akin to aiming at a moving target. The best advice to students might be contained in Trachtenberg's chapter on the critical analysis of historical texts. Students as well as senior scholars should read the latest literature in international history with the same kind of meticulous care Trachtenberg recommends for the classic works of A.J.P. Taylor or Fritz Fischer. Some of them will open up new avenues for future research, others will not hold up to expert scrutiny. Yet they will certainly help expand students' (and senior scholars') understanding of the nature and practice of international history.

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Review by Geoff Roberts, University College Cork

Marc Trachtenberg's modest aim is 'to provide a practical guide to the historical study of international politics—a guide to how historical work in this area can actually be done, a guide which people working in this field might actually find useful' (vii). In my opinion he has succeeded brilliantly. Reading his book I was transported back four decades to the excitement I felt when reading C. Wright Mills' appendix 'On Intellectual Craftsmanship' in *The Sociological Imagination*.[1]

Professor ***Geoffrey Roberts*** *teaches History and International Relations at University College Cork, Ireland. A specialist in Soviet foreign policy, his books include:* The Unholy Alliance: Stalin's Pact with Hitler *(1989);* The Soviet Union and the Origins of the Second World War *(1995);* The Soviet Union and World Politics, 1945-1991 *(1998); and* Victory at Stalingrad: The Battle That Changed History *(2002). His most recent book is* Stalin's Wars: From World War to Cold War, 1939-1953 *(Yale, 2007). He also edited* The History and Narrative Reader *(Routledge, 2001).*

While Trachtenberg's use of Pearl Harbor as the central case study of his exploration of *The Craft of International History* is excellent, I do not find some of his other examples so convincing. In particular, I do not agree that the absence of evidence in western archives that the Soviets wanted the re-establishment of a united Germany demonstrates that this was not Stalin's goal in the immediate postwar period (158-159). In fact, the creation of a united Germany—provided the terms were right—was the consistent aim of Soviet policy from the Potsdam conference onwards (prior to that Stalin had indeed favoured dismemberment). There is plenty of evidence in western sources of Soviet proposals and overtures regarding German unity; and the new materials from Russian archives simply confirm that this was a real policy and not just propaganda. Indeed, my own detailed research on both sets of sources has convinced me that the Soviets did not finally abandon the goal of a united Germany until the end of 1955.

But this particular argument about evidence and its interpretation can wait for another day because Trachtenberg's book is much more than a masterly exposition of the historian's craft. It is also an important contribution to the developing dialogue between historians and political scientists working in the field of International Relations (IR). One of the features of this discussion has been the effort by practising historians to define the theoretical basis of their research and how it compares with the disciplinary traditions of political scientists.[2] Trachtenberg—a diplomatic historian who is Professor of Political

[1] C. Wright Mills, *The Sociological Imagination* (New York: Oxford University Press, 2000 edn) 195-226.

[2] See especially the contributions of John Gaddis and Paul Schroeder to Colin Elman and Miriam Fendius Elman (eds), *Bridges and Boundaries: Historians, Political Scientists and the Study of International Relations* (Cambridge, MA: MIT Press, 2001) 301-326, 403-416. For my own contribution to the discussion, see Geoff Roberts, 'History, Theory and the Narrative Turn in IR', *Review of International Studies*, vol. 32, no. 4, 2006, 703-714.

Science at UCLA—devotes his first two chapters to this topic and it is this section of the book that I want to focus on in my contribution to this roundtable.

Chapter one on 'The Theory of Historical Inquiry' is devoted to the nature of historical understanding and explanation, and Trachtenberg begins by contrasting the views of Carl Hempel and R. G. Collingwood. The philosopher Hempel published a famous article in 1942 on 'The Function of General Laws in History' in which he argued that for an historical explanation to be anything more than a mere 'explanation sketch' it had to demonstrate that what happened was inevitable given an initial set of conditions and the operation of a 'covering law'. In other words, in history as in science, explanation should take the form of a deductive theory (1-4). As Trachtenberg notes, Hempel's views were not very attractive to most historians, who thought that covering laws were notable by their absence in human history and considered historical events more a function of human choice and agency than of deterministic processes. However, as he also says, the Hempelian approach does have the virtue of underlining that 'a strong interpretation should have a certain predictive force'; that is, if further evidence confirms the expectations generated by an historical argument then that is a good test of its explanatory power (4).

The main philosophical alternative to Hempel when the 'covering law' debate was at its height in the 1950s and 1960s was offered in Collingwood's work on human action and the re-enactment of past thought. According to Trachtenberg, 'the Collingwood theory was quite extraordinary. … [T]he historian was concerned not with events as such but with actions' (4). Later he asserts that Collingwood's 'whole approach would today … strike even the most conservative historians as narrow and dogmatic and in fact as a bit bizarre' (5). Consequently 'for most historians the Collingwood theory was not taken too seriously' (6). Hempel and Collingwood, states Trachtenberg, 'represented opposite ends of a spectrum: one emphasized structure and law-like regularity, and the other free will and human agency. But every practicing historian knows that both factors come into play. Part of the art of doing history is being able to figure out how exactly in any particular case the balance between them is to be struck, and this of course is an empirical and not a philosophical problem' (6-7).

Collingwood, who died in 1943, was an historian (of Roman Britain) as well as a philosopher. On this side of the Atlantic, at least, he is known as the historians' philosopher. I would see myself as a neo-Collingwoodian historian and I do not think of myself as particularly reactionary or bizarre. More pertinently, despite his dismissal of his utility, I would classify Trachtenberg as a Collingwoodian too! True, Collingwood thought the primary object of historical knowledge was past human actions, but that did not mean he discounted either the importance of the events created by those actions or the impact of past actions on the shape of subsequent events. Although Collingwood was mainly concerned with the impact of intentional, rational action and described the historian's reconstruction of that action in terms of a psychological process of 're-enactment', that did

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not preclude a role for the irrational, the unforeseen and the accidental, or, indeed, the force of material circumstance.[3]

As far as I can see, each and every case study in Trachtenberg's book is classically Collingwoodian in its focus on the reasons for the human actions that led to major historical events, whether Pearl Harbor, the outbreak of the First World War or lesser occurrences such as the Skybolt affair. The same applies to most historical research, not least in the sphere of international history, including Trachtenberg's own writings. I detect little determinism, except in the loosest sense, in any of Trachtenberg's books, but see plenty of evidence of how human free will, choice, decision, and action determined what happened.

One point on which Trachtenberg does agree with Collingwood is the method of posing questions about the past that are then put to the empirical test of evidence (15-16). In this respect the methods of historians are analogous to those of scientists. In both cases the object of the exercise is to detect a meaningful pattern of events that enhances our understanding of either nature or human affairs. From a Collingwoodian perspective, the events that constitute human affairs are primarily a consequence of human action—a point of view that Trachtenberg implicitly endorses in his practice as an historian, if not in his explicit theorising.

A third theorist tackled by Trachtenberg is Hayden White (7-11). It is in this section of the book that he comes closest to dealing with what for me is the missing dimension of *The Craft of International History*: an explicit treatment of the question of narrative. Narrative is the typical mode in which historians, especially diplomatic historians, choose to conduct their research and write up their results. Their explanations and understandings are embedded in the stories they tell about past actions and events. Historians' narratives are often replete with analyses, generalizations and interpretations, but it is the storytelling core that constitutes both the form and the content of historical knowledge. Hayden White's well-known take on historical narration is that these stories are artificial forms imposed on the past by historians. To that extent the 'poetic acts' of historians that create narratives result in 'verbal fictions' that are as much invented as found in the evidence. Trachtenberg responds to this challenge by defending the idea that historical research does connect up with the real world and does produce meaningful knowledge that is in some sense true (if not The Truth).

This defence is fine as far as it goes, but an alternative account of historical narrative to that proposed by White would have been welcome, and many such accounts do exist.[4]

[3] In his commentary on Collingwood Trachtenberg refers to *The Idea of History,* a posthumously published composite text of his writings, lectures and manuscripts. A clearer and perhaps more authentic guide to Collingwood's views can be found in the later edition of his work, *The Principles of History* (Oxford: Oxford University Press, 1999).

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Trachtenberg prefers instead to utilise the writings of the philosopher of science N. R. Hanson to argue that the essence of historical understanding is to identify patterns and see how things (events) fit together (14-29). But what creates the pattern and the 'fit' in the first place? Surely it is human action. Narrative is generally preferred by historians because it best captures the patterning and interconnections created by human action. This applies to Trachtenberg's work as much as it does to my own and *The Craft of International History* is an edifying exploration of the many techniques that historians use to construct their narratives. Missing for me, however, is a recognition that form flows from content and that historians narrate action not just because that is what they find in the evidence: it is their fundamental way of looking at the world, their ontology as the philosophers would say.

In chapter two Trachtenberg considers 'Diplomatic History and International Relations Theory' and asks what historians and political scientists can learn from each other. His answer is that political scientists can generate interesting questions for historians to test empirically, while political scientists can learn from historians a good sense of the way the world actually works. In common with most international historians (myself included) Trachtenberg is what the IR people call a 'soft realist': broadly speaking, this entails the view that states and their governments pursue perceived national interests as and when they can, depending on power calculations and domestic politics. Within this framework of thinking about international politics there is also ample room for the role of ideas, personalities and the particular choices of politicians in specific circumstances. You could call this broad sense of how international politics works a theory, as Trachtenberg seems to want to, but to me it is just commonsense, informed by experience and rationality. How else would you expect states to behave in the given historical circumstances of international politics? On the other hand, Trachtenberg has a point when he quotes Robert Jervis to the effect that it is only having an explicitly formulated theory in the form of generalizations about expected patterns of state behaviour that makes it possible to be surprised by the course of events, opening up the possibility of challenging cherished assumptions about the way the world supposedly works (38). In the end it is this element of surprise that makes the detail of history so interesting.

The rest of the book is devoted to instructing historians and political scientists alike in methods of documentary analysis and the thinking through of interpretative problems. Trachtenberg's reflections on these aspects of his craft are as practical as they are insightful and illuminating. We all have much to learn from him, not least from his final chapter on 'Writing it Up' where he tellingly glosses Peter Gay's injunction that style 'is not the dress of thought but part of its essence' (183). Trachtenberg here returns also to Hanson and to the notion that the aim of historical texts is to interpret and structure events in order to reveal the logic and meaning of past patterns of human affairs. Trachtenberg links this to the idea that one of the keys to historical reconstruction and understanding is appreciating 'a

[4] Most importantly, see David Carr, *Time, Narrative, and History* (Bloomington: Indiana University Press, 1986); Frederick A. Olafson, *The Dialectic of Action* (Chicago: University of Chicago Press, 1979); Dale H. Porter, *The Emergence of the Past* (Chicago: University of Chicago Press, 1981).

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certain element of *necessity*' in an unfolding story, which he also calls 'the basic *logic* underlying the course of events' (185). This is the point at which I part company from him. From my narrativist and actionist perspective, a quite different emphasis emerges: it is the element of the *possibility* of human choice that prevails, not the logic of events.

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Author's Response by Marc Trachtenberg, University of California, Los Angeles

What was my goal in writing the book that's the subject of this roundtable? I wasn't trying to provide a kind of "blueprint for research projects in international history." I wasn't even particularly interested in sorting out the philosophical issues I dealt with in the first part of the book—at least not as an end in itself. My focus was on the nuts and bolts. I thought of myself as a kind of plumber or electrician who had been practicing his trade for over forty years and wanted to help people who were just starting out learn a bit about how this sort of work is actually done.

Marc Trachtenberg *got his Ph.D. in history at the University of California at Berkeley in 1974. He then taught for 26 years in the History Department at the University of Pennsylvania. For the last seven years, he has been in the Political Science Department at UCLA. He is the author of a number of books and articles dealing mostly with 20th century international politics, most notably a book on the Cold War*: A Constructed Peace: The Making of the European Settlement, 1945-1963.

I wasn't deliberately trying to push a particular view of how the history of international politics needed to be studied, but I can certainly understand why the book came across to some of the commentators as a defense of a pretty old-fashioned way of doing this kind of work. I do believe that the issue of war and peace is of absolutely central importance, and that the question of what makes for war has to lie at the heart of this whole field of inquiry. Since states make war, that means that you need to try to understand, in the first instance, what states were actually doing. And to understand policy, you of course need to study the archival sources.

But we all know that that's just the beginning. Petra Goedde is absolutely right when she talks about the importance of using all kinds of non-archival sources: "popular magazines, newspapers, film, novels, and non-fictional monographs." It's very important to study these kinds of sources if you want to get a feel for how the larger society dealt with a particular issue, and I've certainly used this kind of material in my own work.[1]

The same kind of point applies to the sorts of issues people in our field should be concerned with. As John Ferris points out, military and intelligence issues are of fundamental importance, and sometimes you need to study domestic politics or understand what was going on in the economic sphere. But as I see it—and maybe this is where I part company with some other people in the field—we study these things not just because they're interesting in themselves. We study them because they have a certain bearing on the great issue of war and peace.

[1] See, for example, my article "American Thinking on Nuclear War," in C.G. Jacobsen (ed.), *Strategic Power: USA/USSR* (London: Macmillan, 1990).

What's the justification for this kind of approach? It seems to me that we'd want the field to have a certain intellectual coherence. That means that we need some way to separate the wheat from the chaff—some common standard, some common understanding of what the field is about. A field (if I can paraphrase something Kenneth Waltz once said) "has to be about something. It can't be about everything."[2] That doesn't mean, however, that only a certain type of work, old-fashioned diplomatic history, will do.

And it doesn't mean that the only way to do work in this field is to look at the documents and expect a single, correct understanding of the past to emerge more or less automatically. Professor Goedde says that I offer a "fact-and-documents-based positivism," and that the "book could be read as a spirited defense of what Mary Fulbrook has labeled 'source fetishism' and 'archive positivism.'" There probably was a time when I would have accepted the idea that if the evidence was rich enough you could develop a decent understanding of what was going on pretty much as a matter of course, just by reading the sources and without any great intellectual effort on your part. But one of the main things I've learned over the years is that historical analysis, if it is any good at all, just does not work that way. The facts, as N.R. Hanson said, do not speak for themselves. You have to approach the sources in a very active way. And that means you need to bring a certain conceptual framework, a certain sense for how things work, into play.

That brings me to the question of realism. Is it true that I've bought into the realist world view to the extent that some of the commentators seem to think? I could say that when I talked about how you can draw on a realist understanding of international politics when you're doing historical work—and I did that a lot—I was just giving an example of how a theory could be brought to bear as an instrument of analysis. I could say that I took it for granted that if some other approach works for other historians, then they should certainly feel free to use it. But that answer would not be totally honest, and in raising this issue the commentators were certainly on to something. I do view realism as something special, and for me what makes it special is that it's more than "just commonsense informed by experience and rationality," to use Geoff Roberts's phrase. It's special because, for me at any rate, it has a certain bite that other theories lack: it generates expectations in contexts where other approaches don't lead you to expect much of anything in particular. This is important not because those expectations are necessarily borne out by what the evidence shows. It's even more important in cases where those expectations are not borne out, because what happened in those cases then strikes you as puzzling, and a puzzle can play a very important role in driving historical work.

Consider, for example, the case of the Cold War. I'm a child of that period, and I grew up thinking the Cold War was very easily understood. Of course liberal America would be at loggerheads with the Soviet Union: the ideological gap was so wide that the two sides were bound to be at odds with each other. But then gradually, over the years, I came to look at international politics in a very different way. Without even understanding what was going

[2] "Interview with Ken Waltz," *Review of International Studies*, vol. 24, no. 3, 1998, 379.

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on, just by studying great power politics in Europe in the late 19th and early 20th centuries, I somehow absorbed a rather different view, a view in which power factors loomed large. And when years later I started to think about the Cold War, that whole way of looking at things naturally came into play. The U.S. government might not have liked what the Soviets were doing in Eastern Europe, but it was not going to go to war to prevent them from Communizing the area. And the Soviet government, for its part, might not have liked the political system that the Americans and their friends were setting up in Western Europe, but it was not going to try to force the Americans out of that part of the continent. Given those realities, it seemed that both sides were more or less locked into the status quo, but if so, where was the problem? Viewed in this light, the Cold War was profoundly puzzling. The goal now was to unravel that puzzle; this could give your work a kind of focus which it otherwise would not have had. But you wouldn't have understood that the puzzle even existed if you hadn't brought a certain theory, a certain sense for how things worked, to bear on this issue.

I'd make the same point about 1941. You begin with certain basic observations about Germany and Japan at that time. Here's Germany, in the latter part of that year, fully engaged in a war with Russia, and here's Japan, heavily involved in a war with China. In such circumstances, why would they go to war with the United States, by far the strongest country in the world in terms of potential war-making power, if they could at all avoid it? Approaching the issue from a realist point of view doesn't give you any answers, but you do get some sense for the dimensions of the problem. You're not assuming that Japan's and Germany's decisions to go to war with America were necessarily rational, but you're reluctant to dismiss their actions too quickly as utterly irrational. And that's just the beginning of a thought process. All it does is suggest that the Americans might not have been as passive as some writers might have led you to think.

So by bringing that kind of thinking to bear you've set a thought process in motion. You need to understand what American policy was in 1941. What do you do at that point? Should you just go along with what other people say? In a sense, it's very hard not to. When your own conclusion "is out of line with that of most other historians" (as Antony Best says), it's hard to not feel that you must have missed something, that there must be something wrong with your whole line of argument. And yet can you really just accept what other people say on faith? Isn't there some way to reach conclusions that you have real confidence in?

I think that if you want to make up your mind for yourself, there is a way to proceed, and that's by first trying to understand the structure of the problem and then by focusing on those issues that you now see as fundamental. In the 1941 case, perhaps the most basic issue has to do with the oil embargo. Did President Roosevelt understand the implications of the embargo when it was imposed? Did he believe the embargo would lead to a Japanese attack on the Indies, and if so, did he think that that would lead to a U.S.-Japanese war? And you try to answer those questions not by looking mainly at historians' opinions, but by focusing on the evidence they present. I think it's very clear the president thought an

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embargo would lead Japan to attack the Indies (which is why he had opposed that measure so strenuously up to that point). I think it's also a safe assumption—but I stress that word "assumption"—that he did believe the United States would be brought into the conflict if the Indies were attacked. After all, the U.S. government had set policy for the western powers as a whole; the Dutch and the British had basically followed the Americans' lead; in such circumstances, the U.S. government could scarcely leave its European friends in the lurch if Japan attacked their possessions in the Far East. Given the seriousness of the issue, it's hard to imagine that Roosevelt could have avoided the question of what the United States would do if Japan continued to move south, and it's hard to imagine that he concluded that America could keep out of the war. But in spite of all that, Roosevelt opted for a hard line in late July: the embargo policy—and the evidence on this point is quite compelling—was his policy; it was not put in place by his subordinates acting on their own. And putting two and two together you reach an important conclusion, important precisely because it's at odds with the conventional wisdom in this area: namely, that Roosevelt was deliberately putting the United States on a collision course with Japan.

This still strikes me as a relatively solid argument, and none of the comments convinced me that there was anything wrong with this line of reasoning. But you can't just reach that conclusion about Roosevelt and leave it at that. For if the president had in fact opted for a policy of that sort, that policy choice would be quite puzzling, especially if you think (as I do) that he was moving toward intervention in the war against Germany. Why was he willing to risk a second war with Japan by opting for the embargo and the whole policy that was associated with it? It's hard not to deal with that issue, but the analysis of this question—not just mine, but anyone's—is necessarily somewhat speculative. No one knows for sure what was in Roosevelt's mind at the time, and the methodological issue here has to do with how you form an opinion—that is, an assessment of how likely it is that a particular interpretation is valid—in cases where the direct evidence is rather thin.

The answer I tried to give is that you cast a wide net, you look for indirect evidence, you try to imagine, given what you know about the situation and about the personalities involved, what the thinking "must have been." In doing this, it's true, I personally tend to lean toward rationalistic interpretations. But I don't think that in doing so I'm being totally arbitrary. In the case of Roosevelt in 1941, given the seriousness of the issues he had to deal with, and given the kinds of arguments the people around him (especially the military leaders) were making at the time, I think he was under enormous pressure to think these issues through as well as he could, and a rationalistic interpretation is based on that premise.

This does not mean that other interpretations are not possible, although they too are based on assumptions, although perhaps less rationalistic ones. All of which brings me back to the question of "interpretive pluralism." I certainly agree with Petra Goedde that "there is no single way to write the history of the Second World War." In fact, I doubt whether any two scholars would write it the same way. But that, of course, does not mean that all interpretations are equally valid. Many questions—the question, for example, of whether Roosevelt wanted to take the United States into the war with Germany—are answerable in

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principle, and some questions, I think, are actually answerable in practice. The idea that you should try to answer those questions that are answerable, the idea that you should try to see how interpretations stack up against each other—those ideas are perfectly consistent with a certain kind of pluralism. They're based on the notion that other people's arguments deserve to be taken seriously—that they deserve to be analyzed with some care—and that notion, it seems to me, lies at the heart of a truly pluralistic intellectual world. I don't think we'd want to come anywhere close to embracing an "anything goes" philosophy. I don't think we'd want a world in which people feel they can't make judgments about whether an argument stands up in the light of the evidence, a world in which people feel it's somehow improper even to try to get to the bottom of a question. George Smiley, in a Le Carré novel, once spoke about the "half-baked tolerance that comes from no longer caring."[3] I think we should aim for something better than that.

That's why I feel that exercises like this roundtable are so important in the life of our discipline. The commentators have raised some serious issues, not just about the book I wrote but about how history in general is to be done. It's true that I approach the subject in a particular way. They understood what I was saying and reacted intelligently and perceptively to what they saw in the book. The problems they pointed out—above all, the problems related to a (perhaps excessively) rationalistic approach to history—are problems I've been struggling with myself for some time. These are difficult issues, even for the practitioner, and I wouldn't claim to have a monopoly of wisdom in this area. But I do think it's important to talk about these issues, and I'm very happy that we've had a chance to discuss them here.

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[3] John Le Carré, *Tinker, Tailor, Soldier, Spy* (New York: Bantam, 1990 edn), 80.

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